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## DEATH AND INDIVIDUALITY.

THE current conceptions of death as a biological phenomenon are very confused and unscientific. In this essay I shall endeavor to analyze the problem, and, by placing the factors concerned in a clearer light, to diminish the obscurity in which the subject is still involved. This appears to me the more desirable, because the recent publications of Weismann and Goette upon this general topic have increased rather than lessened the existing confusion. In fact, these authors fail to make the necessary distinctions between the different kinds of death, the different orders of individuality, and the different forms of reproduction. This assertion is, I believe, justified by the following paragraphs :—

First, as regards individuality. Individuality, as it is generally understood (i.e., as something always equivalent to itself), does not exist in nature, except subjectively as a rather fantastic notion of the human mind. The term 'individual' is applied to things utterly incommensurate with one another. An individual protozoon, an individual polyp, and an individual insect, are not homologous and comparable bodies. It is mere slavery to a false form of speech to imagine that their 'individuality' is a common quality; for, on the contrary, the same word indicates here three distinct phases. I know not how to account for the immense significance attributed to the mystical idea of individuality, which in reality corresponds only to a physiological capacity for a separate existence, but in usage is tacitly assumed to be the name of some vague fundamental property of life, which, however, the mind cannot apprehend. Now, we have renounced considering a wing in a bee, a bird, or a bat, as identical or homologous with every wing, either on account of its name or its function. But, although the different kinds of individuals of animals and plants are much more unlike one another than are the manifold types of wings, yet individuality is generally taken to mean a uniformly identical something; and that is untrue. Of course, the matter is really very simple, and indeed self-evident, as to its true nature; and the singular obscurity prevailing is probably due only to the problem not having been clearly thought over. At present the condition of opinion upon the subject reminds one of the ancient notions of beauty, according to which, beauty was an inherent quality of objects, not an impression of the mind, a psychological state. Despite custom, it is plain that 'individ-

ual' has many meanings; yet it is usual to compare 'individuals' with one another throughout the animal kingdom. This error has been repeated by Weismann and Goette, because they both assume that the death of a single protozoon is equivalent to the death of one of the higher animals. Goette, however, has partially emancipated himself from this idea, which I believe to be erroneous. The death of a unicellular, is entirely different from the death of a multicellular, individual.

To Huxley<sup>1</sup> we owe the first scientific determination of individuality. His essay on the subject ought to be thoroughly studied by every biologist. Life occurs in cycles of cells; each cycle comprises all the cells springing from a single impregnated ovum; the whole of every cycle is homologous with every other whole cycle, no matter whether every cell is a so-called individual, or whether they constitute several individuals (e.g., polyps) or a single one (vertebrates). All cells are homologous, all cycles are homologous; but individuals are not always homologous, since an individual may be either the whole or any fractional part of a cycle. This question I have discussed a little more fully on pp. 191, 192, of my article cited in the footnote.<sup>2</sup> Manifestly the death of the single cell is not necessarily identical with the termination of a cycle. Now, when a man, he being a cycle of cells, has lost the ability to continue the cycle, he (or it) dies. Further, it is inherent in his constitution to lose that ability gradually: hence, when it is completely lost from internal causes, he dies, as we say, from old age. It is to this ending-off of the cycle, from causes resident in itself, I wish to restrict the term 'natural death.'

We have now two questions to pose: 1°. Do all organisms belong to cell-cycles? 2°. If so, are all cycles self-limited? In common language, the second question would be, Is death always the natural and inevitable accompaniment of life? — an inquiry which may appear singular, but is none the less perfectly sensible and legitimate. Weismann has answered it with a negative.

1°. I maintain the hypothesis that all organisms do develop in cycles, and only in cycles; which involves the assumption that all living species begin their life-history with an impregnated ovum or its equivalent. We come, therefore, at once to the question of

<sup>1</sup> T. H. Huxley (1852) upon animal individuality, *Royal inst. proc.*, i. 184-189; *Edinb. new phil. journ.*, lxxi. 172-177; *Ann. mag. nat. hist.*, 1852.

<sup>2</sup> C. S. Minot (1879), Growth as a function of cells, *Proc. Boston soc. nat. hist.*, xx. 190-201.

how far sexual reproduction extends downward in the scale of life. I deem it very probable that it extends to the lowest animated being, even though it be quite differently manifested in the lower forms from what we observe in ordinary bi-sexual reproduction. This view is opposed to the opinions generally held: for botanists trace the evolution of sex within the vegetable kingdom; and zoölogists trace it, though less definitely, within the animal kingdom. We are thus forced to assume that sex, one of the most fundamental and characteristic phenomena of life, has arisen twice. This is to the last degree improbable. Such a coincidence would be the most extraordinary result of chance within human experience. It is more reasonable to suppose, that, though we do not yet recognize it, the sexual function exists in the probiotants, which are neither animal nor vegetable, and that they also produce a body homologous with an impregnated ovum; and to suppose, further, that, out of this common commencement, both animal and vegetable sex have been evolved. The essential property of the sexually produced ovum is its power of repeated division, producing a succession of cell-generations, which, together with the original body (*ovum*), constitute the cycle. There is much evidence of a positive character to confirm the belief of the cyclical course of life, even among the protozoa and protophytes, in which there occurs what is known as rejuvenation (*verjüngung*).

2°. I maintain that it is probable that all cycles of cells are self-limited. Let us first ascertain the nature of the limitation. Our knowledge of the manner in which the cycles are limited (i.e., of the causes of natural death) is very restricted, and derived solely from the higher animals. My own special investigations have been in this field, and have led me to the opinions and problems we are discussing.

My experiments demonstrate, that, when properly analyzed, the growth of at least the higher animals gradually diminishes from birth onwards, almost without interruption. This is an irrefutable mathematical verification of the views which I advanced in my article on 'Growth as a function of cells,' published in 1879, the essence of which, as far as we are now concerned, is, that the cells of a cycle continuously lose their power of division, so that the interval between two successive divisions gradually increases. This involves the ultimate termination of the cycle, because the losses go on, not only until the cells can no longer divide, but until they exhaust them-

selves. This whole series of changes is properly *senescence*, or growing old. Senescence is a continuous process, covering the whole period of a cycle of cells; and we must assume it is the positive loss of power in the single cells, such that the last-produced cells cannot continue, and *natural death* ensues. Of course, in the cases of a multicellular animal, death of the whole follows secondarily upon exhaustion of any essential part; as in the case of insects, which die upon laying their eggs. In the higher animals, then, the cycle is limited by senescence, and senescence is a decay which probably begins when the cycle begins. The next point to decide is, whether the same phenomenon occurs with the unicellular organisms. If it is found that the divisions of a *Paramecium*,<sup>1</sup> for instance, after a conjugation, are at first rapid, and then follow at increasing intervals, it would prove (provided, always, the external conditions remained constant) that we here had true senescence, with its sequel, natural death, or the end of the cycle. Until this point is settled, we cannot know whether there is, among unicellular animals, a form of death homologous with the natural death from senescence in the higher animals and plants.

It is to be regretted that both Weismann and Goette appear not to know the article to which reference has just been made: otherwise they would have recognized that the problem of death is, *first*, whether growing old (*veral-tung, involution*) is a universal phenomenon of life. Weismann's first article was an address delivered before the German *naturforscherversammlung*, September, 1881, and subsequently republished at Jena.<sup>2</sup> He advanced then the view, that, for unicellular organisms, there is no death except through accident; that, the propagation being by simple division, we must assume that the process of division may go on forever. He does not even consider whether the cells form cycles, and whether these cycles need to be renewed; so that he misses the real problem. On the contrary, he is enthralled a prisoner to the mystical idea of individuality, and reasons as if individuality rendered direct comparisons legitimate between things essentially different. All his reasoning is based upon the idea that an individual protozoan is comparable to an individual dog, and so on. The argument just made against him was to show that the basis of his whole fabric is illusory. Bütschli, in his short article,<sup>1</sup> called forth

<sup>1</sup> *Paramecium* is a common unicellular animal.

<sup>2</sup> Weismann, *Ueber die dauer des lebens* (Jena, 1882, 8°, 94 p.). Cf. also Weismann's comments on Bütschli, *Zool. anzeiger*, v. 377-380, and his reply to Goette,—*Ueber leben und tod* (Jena, 1884, 8°).

by Weismann's, partially liberates himself from the confusion as to individuality, and propounds the hypothesis of a *lebensferment*, which he supposes to be continually renewed in protozoa, which he thus assumes to be potentially immortal. He also fails to recognize that the true question is, not whether single protozoa die, but whether they form senescent cycles. In this error he is followed by Chodowsky,<sup>2</sup> who also admits that natural death is restricted to the multicellular animals, but overlooks what would be its only possible homologue among protozoa.

Goette seems to me to have made a distinct advance beyond his predecessors, for he has attempted<sup>3</sup> to show that there is a death common to all organisms. Especially is his conclusion that death and reproduction are intimately connected to be noted as important; but his thought appears to me often vague and obscure, and to many of his views I can by no means assent. I have just asserted that death and reproduction are intimately connected. Now, if my theory is correct, it is evident that each cycle, before it is completely exhausted, must produce the initials of new cycles: hence the connection in time between maturity, or the approach of death, and sexual reproduction. By speculation upon the few available facts, I have reached the following hypothesis. Originally each cell of a cycle was a distinct individual; the exhaustion of the last cells of the cycle caused them to become sexual bodies and to conjugate; conjugation renews the power of division in the conjugated individuals, and therewith a new cycle is begun. Subsequently multicellular animals were evolved, and in these the same phenomena recur; but some of the cells have become specially organized, and thereby incapable of assuming the sexual state: hence, when the end of the cycle approaches, only a few cells become sexual, and the animal (or plant) is mature. The higher organisms become sexually active only after having grown for a considerable period, because they still preserve the primitive relation. Senility is the *auslösende reiz* of sexual reproduction. I hope to discuss the matter fully in a memoir which I am now preparing for the press.

It is evident, that, according to this hypothesis, sexual reproduction depends on the exhaustion of the cells. There are many facts known to confirm this view. Thus among men

<sup>1</sup> O. Bütschli (1882), *Gedanken ueber leben und tod*, *Zool. anzeiger*, v. 64-67.

<sup>2</sup> N. Chodowsky (1882), *Tod und unsterblichkeit in der thierwelt*, *Zool. anzeiger*, v. 264, 285.

<sup>3</sup> A. Goette (1883), *Ueber den ursprung des todes* (Hamburg and Leipzig, 1883, 8°), p. 81.

the reproductive period begins sooner when they are ill fed. Among many of the lower plants, reproduction is induced by defective nutrition. I believe that nutrition and reproduction are, indeed, opposed to one another, but by no means in the sense taken by Carpenter<sup>1</sup> and Spencer.<sup>2</sup> While I consider that the impaired nutrition causes the effort to reproduce, they believe that reproduction is opposed to nutrition, constituting a tax which withdraws just so much from the parent. Undoubtedly, in those cases where the parent, in consequence of a secondary addition to the office of genesis, has to supply food to its young, reproduction may detract from growth, but, even in such cases, only sometimes. Carpenter and Spencer's whole argument rests upon the assumption that the power of assimilation is only just equal, or about equal, to the demands of the parent. It is, however, perfectly well known that the reverse is true, and that there is in most organisms a large surplus of assimilation possible, which is used whenever the functions demand it: hence in most cases the secondary taxes of reproduction can be wholly or mainly paid without calling on the growth capital of the parent. Spencer's *a priori* argumentation I consider superficial: it has led him to an exaggerated idea of an opposition which exists in nature, but is not general. Moreover, Spencer has mistaken the cart for the horse: animals do not stop growing because they begin to reproduce, but they begin to reproduce because they stop growing; or, more strictly speaking, both events are due to one cause,—senescence.

It will be seen, upon reviewing the preceding paragraphs, that the views I advocate are opposed to all the other opinions upon the nature of death which have been noticed above. In a memoir I am now at work upon, I hope to array a large number of observations to defend the theory outlined in this essay.

C. S. MINOT.

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#### AMERICAN APPLIANCES FOR DEEP-SEA INVESTIGATION.

##### The wire dredge-rope.

It was a revolution in deep-sea dredging methods, when the cumbersome hempen rope was discarded for one of wire, measuring scarcely more than one-third the same diameter, stronger, more durable, and less expensive. The introduction of wire-rope will not affect

<sup>1</sup> William B. Carpenter, *Principles of physiology, general and comparative* (3d ed., 1851), p. 592.

<sup>2</sup> H. Spencer, *The principles of biology*, vol. ii. pt. vi.